THURSDAY, APRIL 29, 1880

GEODESY

Geodesy. By Col. A. R. Clarke, C.B., R.E., F.R.S., &c. (Oxford: Clarendon Press, 1880.)

IT is well that there are men brave with the pen as there are others brave with the surgeon's knife or the soldier's bayonet—to whose actions the word temerity can only be applied in the full consciousness that the moving force which impels them is neither vainglory nor ignorance, but a strong sense that providence or fate has placed them in a position where, and when, they, and they only, must obey that call which they feel to be the duty of the moment.

It is not often that this call is so clear to the literary and scientific ear as it has come to be in the province marked broadly on the map of knowledge as GEODESY. And certainly there are few men in England to whose ear we may believe that such a call could have been more directly addressed than to that of the author of this work. It is well, we say, that he has bravely attended to it.

The work to which Col. Clarke has set his hand and seal was one to be dreamt of rather than executed, and we doubt if the worst we could say of the finished deed would not find something more than an echo in the mind of so careful an author. For in truth the task was one of very serious difficulty; and it is in no depreciatory spirit, but quite the reverse, that we have to recognise, by their absence, the chapters—we might almost say the volumes—on various branches of the great subject which we cannot help looking for in a work bearing so broad a title as "Geodesy."

It would be difficult, if not impossible, to treat such a subject as geodesy in a manner calculated to enlighten the present generation as to the present position of the questions which it has raised, without constant reference to the stages through which it has passed. Cousin germane to astronomy, it may claim to be treated with some measure of the respect which has given rise to so many histories of that science; but geodesy can as yet boast no historian. It is a strange fact, and the reason of it is by no means so obvious as the fact is to be regretted. One immediate consequence is that a writer eminently competent to write a treatise on theoretical and practical geodesy is debarred from doing so without the *arrière pensée* of a neglected history—in which department he labours under an obvious pressure of other demands. Thus the first chapter of the work before us reminds one of the explanations which are given to a visitor to some manufactory, who comes out at the end of a series of workshops with a general sense of having had glimpses of interesting work, and a recollection of words having a scarcely understood connection with the processes witnessed; but quite certain, if he paid the same visit a hundred times in the same hurried manner, that he could not understand the manufacture well. There are many points in this first chapter which are either historically inaccurate or so collocated as, while capable perhaps of correct interpretation, to suggest to a mind previously unread more or less erroneous conceptions. It is needless to give instances; for besides that they would but concentrate a needless criticism on points of detail, it is evident that a full account of geodetical operations during two centuries and a half cannot be given in a chapter of thirty-six octavo pages, a very considerable portion of which is taken up with tables, diagrams, mathematical explanations, and numerical examples.

Remarking here, as elsewhere throughout the volume, the author's neglect of the opportunities which are afforded, in the course of a relation of facts, to explain causes of failure, to comment on erroneous and vitiating opinions, as well as to commend those which have borne the test of later experience; in a word, to take upon him the burden of judging wherein lay the foundations of what may now be recognised as sound principle—remarking this abstinence, we are the more pleased to notice and to give some slight additional currency to an exception, which finds expression in at least three places in different parts of the work. It is a condemnation of systems of observation which do not aim at "the 'bugbear' constant error, which is, or should be, the first and last anxiety of every observer." It will be long before a rule, which has all the appearance of courting error rather than truth, will be so generally recognised as to need no "bushing." Meanwhile let it stand forth:—

Presume the existence of a constant effect as the natural concomitant of constant conditions, and if such effect is not itself the object of inquiry, destroy it by opposing conditions, or baffle it by varying them.

At the same time let it be noted that in very many kinds of physical observation, that which is sought depends directly on a *difference* of results, and where this is the case constant errors require to be regarded in a very different light. Geodesy is full of such cases, and one of the most important is to be found in the use of the differential pendulum, where maintenance of condition is the *sine quâ non* of exact result.

The work before us consists of fourteen chapters, each of which is devoted to one particular branch of the subject; and these, on the whole, form a tolerably connected chain of narrative, argument, theory, explanation and illustration, calculation, and discussion of result. Considered as a work on geodesy, it is noteworthy that the last chapter but one (Chapter XIII.) discusses the Figure of the Earth, while the last is devoted, self-contained, to the theory, practice, and results of observation with pendulums. The inference to be drawn from this division is that whatever is to be gathered regarding the earth's form from pendulums is outside of the region of geodesy. So far is this from being our own view, that we would rather have seen this relegated chapter occupying its legitimate historical place as at least the second if not the first in the book. As such it shall be dealt with here.

The mathematics of this part of the subject are very brief and to the point. None of the numerous difficulties are even mentioned here which have at one time or another cropped up, and upon which pages innumerable have been written, printed, and published—to wonderfully little purpose, so far as the practical accuracy of pendulum observations is concerned, but not, perhaps, altogether without influence on collateral physical inquiries. The history of pendulum observations is also very briefly dismissed, with less inaccuracy than commonly falls to its lot. The "invariable" pendulum is of

course described as Kater's, and the ball and wire as Borda's, the opportunity being once more lost of assigning both to their proper epoch, viz., 1735. The method of coincidences is described, but without correcting the common misapprehension of it as a modern improvement, while the introduction of the lens, by means of which Bessel separated the experimental from the clock pendulum by several feet, without impairing but rather facilitating the exact observance of coincidence, is not noticed. In selecting the data with which the recent Russian and Indian observations are to be combined for a new evaluation of the ellipticity, the materials are taken direct from Baily's table, which is open, to say the least, to considerable objection on several grounds. The absolute determinations, both those anterior to Baily's time and those of more recent data, are all tacitly excluded. Granting that this exclusion is not unreasonable, it is impossible not to feel surprise that the existence even of the enormous body of work which is thus passed *sub silentio* is not even mentioned. The recently recognised fact that most, if not all, the modern absolute determinations with the reversion-pendulum are vitiated to a sensible but unknown extent, which can at best be approximately estimated, is noticed, and might have been given as a reason for deferring their use as available data. As it is, we must reckon the estimation in which they are held from their non-appearance here. The same may be said, *mutatis mutandis*, about the older ones, which are without exception impaired, and as yet unavailable on account of the want of the reduction for air-resistance.

We have scarcely approached as yet the subject of this most valuable volume, and already a considerable portion of our available space has been swallowed up. It is in fact as impossible to give a full account of what the author has compressed into its 350 closely-printed octavo pages as it probably seemed to him to give a full and fair account of the enormous amount of work which has been done in all the many countries where surveys have been and are being carried on. Indeed, as of the history so of the present aspect of geodesy, we may say that it is scarcely more than touched on. Whatever is said of the various survey operations in different parts of the world is introduced in the course of description of methods of observing and of reducing, and by way of acknowledgment of sources and of data, or for the sake of illustration. The book does not pretend to give a digest of geodetical operations so much as to declare broadly, and of course in certain respects particularly, what does and what should constitute geodetical practice and theory.

In reviewing, it is above all things necessary to remember that an author has an indefeasible right to frame, arrange, and treat his subject and material as he pleases. But on the other hand the reader has an equal right to be pleased or displeased with the result, and to say in what respects. When, as in the present case, it happens that a very extensive field and a very difficult subject has remained unappropriated by any competent writer for half a century, and is then claimed by a writer who has been before the world as an authority in that connection, his work almost inevitably takes a place which may be said to await it, irrespective of its actual merit. In such a case an undeniable responsibility attaches to the author, and a no less clear onus lies on the reviewer—on the one hand to fall into no errors through carelessness; and on the other to judge fearlessly, rather than add unnecessarily to authority, which already has so much in its favour. If the one is a difficult task, the other is an ungracious one at best.

Passing on now to what is the real body of this work, we find Chapter II. devoted to "Spherical Trigonometry" and Chapter III. to "Least Squares." With regard to these we appeal to what has been said above about the rights of an author and the duty of a reviewer. They are pure mathematics, and we admire them honestly, as deft and elegant condensations of the principal requirements of a geodesist; but not without a sigh, as the truth dawns upon us that this is but the threshold to mathematical labyrinths, of which no plan is furnished, and in which, therefore, we run a fair chance of failing to find the broad paths to which all must keep who would be in accord and who would work together. And in effect in the next chapter we take the desperate plunge into the theory of the figure of the earth, with the method of potentials as our only guide. It is in vain to resist, but, like Christian in the Slough of Despond, we struggle through, with the burden of numberless volumes of National Surveys and Geodetical Operations on our back; and if haply thereafter we meet with a Mr. Worldly Wiseman, surely we will listen to his counsel, and endeavour to find rest without undergoing further like perils.

Towards the close of this chapter we come upon notices of the discussion of twenty years ago as to the possibility of explaining anomalous surface attractions, or the absence of them, by different suppositions as to submontane density. It would be idle to add here a single word to that discussion—not because it has been in any sense closed, so much as because there is room for almost any quantity or kind of fresh assumptions, and because we are unable to see how any decisive result can be reached where all the assumptions are contestable—as for instance that the solid crust is lighter than the plastic nucleus upon which it is supposed to float.

Perhaps the most remarkable result reached in the course of this chapter is the following:—As an example the disturbance of the sea-level is calculated which would be caused by a sphere of matter of surface density one mile in diameter, situated at or near the surface. The calculated amount is found to be only 2 inches; but small as this is, it is further shown that the maximum deflection of the normal to the protuberance thus caused would be 5″ at a distance rather less than 2,000 feet from the summit, or a relative deflection of 10″ between the two points 4,000 feet apart. There can be no manner of doubt that the sea-level surface is not of the same form as would be the case if the mountains were cast into the sea; and if we call the difference "disturbance" of form, then the extent to which the irregularities of form, of whose existence we have abundant proof, is explicable or not explicable by such disturbance is a legitimate part of geodetical study. At the same time, to some it may be a question whether the term geodesy ought not to be understood to consist of the means and methods by which the earth's form and size are discoverable, whatever these may actually be, rather than a general name for all and any studies of the causes or explanations of that form.

Chapters V. and VI. deal with the "Distances, Azimuths, and Triangles on a Spheroid," and with the difficult subject of "Geodetic Lines." By a spheroid is here meant an ellipsoid of revolution of small ellipticity. As, in the sequel, a figure will be sought for which shall, in some respects and for certain limited purposes, serve as a FIGURE OF REFERENCE more closely than any ellipsoid, it may be objected that the term "spheroid," which includes all such *quasi*-spherical forms, should be here qualified by the descriptive adjective "elliptic." And—though it has little to do with these chapters—we may here too remark that the necessity of casting calculations of triangulation into practical forms is the real reason why such non-elliptic spheroids are, and probably must ever remain, unavailable as Figures of Reference on which Trigonometrical Survey calculations can be based. Perhaps all that can be done in that direction is to vary the elliptic spheroid so as to suit the local curvature of special areas of triangulation. But this is anticipating.

The next two chapters are on the "Measurement of Base Lines" and on "Instruments and Observing." Something is said in the earlier about standands. We would gladly have seen a great deal more. Col. Clarke seems often to avoid purposely telling us anything of the origin and meaning of things, and doubtless he would reply to such a stricture that it formed no part of his design. But while bowing to that, one cannot but wish that he had devoted a few pages to giving a concise review of the antecedents of the national standards now in recognised existence. It cannot be that he does not know it all, more thoroughly, perhaps, than any other living person except Sir George Airy. It must be that he is unconscious of the sense of impotent ignorance which so many feel and lament. We know that the Toise, which is now the national standard of Germany, has its prototype in the Toise which Godin copied at the Châtelet, and that the prototype of the Metre bears a definite relation to Godin's—or la Condamine's, as it is often called, because la Condamine procured its recognition as a standard; but what do we know of the Austrian Klafter? and alas, what do we common English folk know of our own yard? Is there not in existence an Act of Parliament defining it in terms of the seconds pendulum? May we affirm that the defining clause was only provisional, and that the whole Act has been repealed? Is it true that our yard is a real entity lying at Westminster; and that there is no other *so* real and actual a standard having a tangible existence? All this we believe—but of what value is such a belief on the part of an unknown reviewer? It is true that Col. Clarke does not leave us wholly in the dark. "The standard yard of this country and its copies," he says, "are bars, an inch square in section, of iron, steel, brass, or copper." There are, we believe, one Standard, five Parliamentary Copies, and some sixty to eighty Secondary Copies—a large number of which are of *bronze*.

In this, as in so many other parts of his subject, Col. Clarke has entirely failed to appreciate the relative worth of information. He is full to overflowing of the knowledge that we want, but there is a part of it which he gives us—not grudgingly we may be sure—but hastily, sparingly, and almost apologetically, as who should say—this we all know; excuse my alluding to it. But in fact all these common things—things of which the want is common to us all—are *not* familiar; we do *not* know them; we find the greatest difficulty in learning about them; and our common knowledge is lamentably defective through the want of them. We hope earnestly that Col. Clarke will recognise this and meet the want in future editions—for that there will be future editions of such a work as this is as certain as that the vast areas being year by year colonised and brought under civilised management will likewise be brought under theodolite and chain in due course. It is with a view to this inevitable extension of surveying operations that we desire so anxiously to see the broader principles of geodesy established on a basis of economy and utility very different from those which have regulated the surveys of older lands.

The chapter on Instruments and Observing is embellished with several excellent representations of theodolites, zenith telescopes, and transit instruments. Though necessarily scarcely more than glancing at the numberless details more or less familiar to the practical surveyor and astronomical observer, it runs through the subject skilfully. We could wish that more were said about the American methods, and we miss at least a notice of the superb alt-azimuth designed by the late Col. Strange—which has never seen service—and his far more successful zenith-sectors, which have both established a reputation second to none. But we are quite aware that nothing less than a series of volumes could do justice to this branch of the subject.

Astronomical observations for the determination of latitude constitute the most important part of the work of a geodetical survey next to those for the determination of distance. These observations consist practically—where a sector is used—in determining the zenith distances of numerous stars. And since these concur to give at any one station but one co-latitude, it follows that they afford a test of the correctness of the N.P. Distances of the stars employed, to each of which is assignable an apparent error, as one of the results of the combination. Where a single station of observation alone is under consideration these apparent errors are rightly enough attributable rather to observation and graduation than to place. But where the same star is thus tested again and again under different latitudes the accordance or approximate constancy of such apparent error can have no rational explanation in any other source than in erroneous N.P.D. This is a practical result which is far from being hypothetical; and we have often thought that neglect to utilise latitude observations in this way with a view to perfect the astronomical place is a waste of material which is well worth attention. The examination here suggested may be readily put to the test of trial by any one who has access to a considerable body of published results of such observations; and if due attention is paid to the identity of the star's place employed at different stations, we can afford to prophesy with confidence that in many cases there will be found sufficient evidence to condemn—and therefore to rectify—the places used.

So much remains to be said in connection with the final chapters that we must abstain from commenting on the next in order—Chapter IX., on the "Calculation of Triangulation," and Chapter X., on "Heights of

Stations," interesting and suggestive as they both are. Few will care to master the former who have not either the misfortune to have a mass of triangulation on hand awaiting reduction, or the luck to have nearly done with one and the curiosity to see whether it is yet open to them to modify their plans. To such as are in the former predicament we may say with the most entire confidence that they will find no safer guide.

In this chapter we remark a short notice of the recent completion of the connection between the Spanish and Algerian triangulations, by a quadrilateral figure whose longer sides, spanning the great inland sea, are 170 miles in length, the longest, we believe, on which luminous signals have been observed. "Thus," remarks Col. Clarke, "a continuous triangulation now extends from Shetland into Africa."

The subject of terrestrial refraction receives ample attention in the chapter on Heights of Stations. We remark as noteworthy that a distinction is found to be necessary between the factor for rays crossing land and those crossing sea We must here also notice one of the very few errors in the book. On p. 261 "the average amount of refraction" is said to vary from $\frac{1}{12}$th to $\frac{1}{16}$th of the arc between the stations. What is meant is no doubt the average amount of *minimum* refraction.

Our task would now be completed by an impartial review of the remaining two chapters on the "Connection of Geodetic and Astronomical Operations" and on the "Figure of the Earth." Unhappily our attitude in presence of these chapters is a prejudiced, though certainly not a hostile, one. We have regarded the earth, mentally, for so many years as an irregular spheroid, and all ellipspids or other mathematically simple figures as mere conveniences that we cannot bring to bear upon the exact determination of any particular one of these that intense curiosity which is necessary to sustain one in the search for "the most probable." Under these circumstances it seems both wiser and more courteous not to contend against views whose only demerit is that we do not sympathise with them, but rather to confess dissent and to offer some considerations from a different point of view.

That the sea-level surface of the earth—by many called, for reasons not very clear, the mathematical surface—is an irregular spheroid, no one nowadays will dispute. Neither is it any longer open to question that an elliptic spheroid of revolution, whose compression at the poles is (say) $\frac{1}{290}$th, is *very like* that irregular spheroid. Let us regard these two things as distinct. We may speak of them as the Earth, and the Form. And we may recognise that the latter is provisional, in the sense of being liable to modification if expedient. If, so prepared, the question be propounded, What is the object of geodesy? the answer must surely be on all hands, to determine the Figure of the Earth, by reference to the Form. *By reference to*, not by confusion with, or by means of, still less by *moulding* the Form until it has ceased to be an elliptic spheroid, and has become, if possible, identical in contour with the actual Earth. The Earth remains the Earth, and the Form remains the Form; and Geodesy aims at determining the want of exact conformity between the two. This is the first consideration.

The next is, How can this be done? The answer clearly is, In the first place it cannot be done at all for the *whole* Earth, by any means at present known; but it can be done *partially* in two ways, viz., by the pendulum, wherever there is *terra firma;* and by surveying instruments where this *terra firma* has ample extension; and, in the next place, it can be done by such and such employment of these implements.

Now it seems to us that it is for the proper comprehension of the scope and bearing of this last instruction that light is needed to be thrown by those who are competent. The interest aroused by pendulum operations, for instance, is almost painfully unintelligent, if we compare the simplicity of the fundamental ideas necessary for its comprehension with the obscurity which has throughout characterised the practical development of those ideas. And though there has been no analogous obscurity in the practical development of the other method, it is not the less true that a shadow of another kind has been cast by something like a misdirection. We cannot study the history of this branch of geodesy without recognising that attention has been constantly directed, not upon the Earth but upon the Form. The whole power of analysis and of calculation has been devoted to perfecting and to designing a "more probable" form, and to showing that on certain conditions (which the earth, if it were only moderately amenable to reason, would recognise the justice of) the form so designed comes very near indeed to what the earth *should be*. What is the inevitable verdict which results from a charge of this description? WE FIND THAT THE MEAN FIGURE OF THE EARTH IS A SPHEROID WHOSE AXES ARE IN THE PROPORTION OF ABOUT 292 : 293. The finding is brief enough, truly; but is it in accordance with the evidence? Surely, yes! for that word "mean" will cover whatever we like to put under it. But it is none the less an unsatisfactory verdict. Let it be remembered that the accuracy insisted on in trigonometrical surveying operations and reductions is far greater than is required for fiscal, commercial, or what are commonly called practical purposes. The object of this exceeding accuracy is geodetical. Thus, for instance, no one would dream of surveying a small isolated island with such accuracy. A great part of the cost of a continental survey, therefore, has to be reckoned as sunk for the sake of ultimately learning more about the exact shape of the earth than we could at present see any direct utility in. But as yet we have got very little further than a positive certainty that that shape is irregular. As surveys extend and get connected with each other, some better return for the labour expended is demanded. The geodesist begins to think of phrenology, and to speculate whether he can yet venture to map out the earth's bumps as he has already mapped out land and sea. He learns to regard what were looked on as local disturbances of the plumb-line as the means to that end. He sees in them no longer mere *errors*, to be herded by the theory of probabilities, but distinct indications of that which he has to work out.

And if, meanwhile, despairing of obtaining, by the slow and grievously costly processes of land measurement, data enough for such a purpose, his eyes should be opened to the practical facility of obtaining such, *ad libitum*, by means of the pendulum; he may well be pardoned if he turns somewhat impatiently away from the

former, and demands that reason shall be shown for not diverting to the service of the latter at least a large share of attention.

Remembering that measurement of arcs and elaborate study of the earth's irregularities by the plumb-line never can extend much beyond the continents and larger islands, and never will extend far in advance of civilisation; while pendulums can, and assuredly will some day, form part of the equipment of every scientific exploring party, it does seem passing strange that we should still be discussing the ratio of the axes of a convenient figure of reference (p. 287) as a more important question than the actual nonconformity of the earth to *some* approximate figure of known form.

What we would fain see, as the *geodetical* fruit of first-class surveys—if not done, then attempted; and if not even attempted, then at least inculcated as to be done or attempted—is, a comparison of the earth's surface, as actually measured, with some provisionally adopted form, showing where possible the relative position of the actual zenith, as determined by astronomical observations, with respect to the formal zenith. And then, a discussion of such results, showing, either a traceable continuity of the irregularities of the actual surface, if such exists; or evidence of discontinuity such as to justify a presumption that the irregularities are too small in area to be susceptible of study without closer distribution of stations.

Thus we might haply arrive at one of two conclusions —that large irregularities exist which may be mapped, or that the irregularities are such as to demand special investigation by a recurrence to observation in selected localities.

If to this suggestion it is objected that the thing has been done—and we know that the irregularity in the neighbourhood of Moscow has been investigated in some such way—we reply that, even so, a short paragraph noticing the fact (p. 288) is but a meagre presentation of what seems to us one of the principal results of methodic geodesy.

And now that we have done and have to lay down the pen, it is with a feeling of regret and a sense of incompleteness. The book deserves so much better than we have said of it. We have identified ourselves too entirely with the student looking for special instruction and too little with the author giving the best he had, and have quarrelled with him because it was too good for our needs. Once more be it said that the subject is too large for a single work—it needs a series. It is but the absence of a few apologetic words that has given this sense of a subject approached at many points only as it seems to be immediately quitted, in favour of others which have more attraction to the author. Now that he has dwelt on them, may he resume his task, and enlarge where we have shown the need.

J. HERSCHEL

OUR BOOK SHELF

The Fauna of Scotland, with Special Reference to Clydesdale and the Western District—Mammalia. By E. R. Alston, F.L.S. (Glasgow: The Natural History Society of Glasgow.)

THE Natural History Society of Glasgow, having resolved to publish a catalogue of the fauna of the western district of Scotland, have secured the co-operation of Messrs. Alston, Young, Cameron, Robertson, Binnie, and Lumsden. Already one part of the catalogue of the Crustacea and one part of the catalogue of the Hymenoptera have been issued, and these have now been followed by the present part, treating of the Mammalia. The Society is doing a good work, and will be fortunate if all the parts as published come up to the standard of the one now before us. In the nomenclature of the Mammals, of which fifty-one are recorded, the author endeavours to reconcile the spirit and the letter of our British Association rules. Without entering into any details of description or economy, he has carefully worked out the geographical distribution of each species. A very interesting list is given of extinct and recent Scottish Mammals, arranged in the probable order of their arrival from the southward.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Auroral Response in America

WHEN the full burst of auroral displays is upon us, and one brilliant demonstration treads close on the heels of another, there may be some trouble in ascertaining which corresponds to which on opposite sides of the earth. But the opening of the new cycle-season by the arc which I described in your pages on March 17, has proved so isolated a phenomenon in time, that it cannot be confounded with any other either before or since. And while your subsequent notice of the disturbance of the magnets at the Royal Observatory, Greenwich, on the same night, proved that the aurora seen in Edinburgh was an earth-ball phenomenon, and not a mere local atmospheric glimmer, the following letter, which has just reached me from Canada, shows a remarkable correspondence to have prevailed there.

The letter, written to me by Lieut.-Col. G. E. Bulger, late 10th (North Lincoln) Regiment, from Montreal on April 10, is word for word simply thus:—

"I have noticed your account (NATURE, vol. xxi. p. 492) of the aurora seen in Edinburgh on the 17th ult.; and it has occurred to me that it might interest you to hear of a similar display which I observed at this place on the same date. Your description would apply very well to the one witnessed here, excepting that the arch was higher in the sky, and its centre about N.E. The darkness below the light was very marked, although the moon was shining brightly at the time. Auroras have been singularly rare here this year, and that referred to is the only one I have seen or heard of since my arrival in August last. The weather on March 17 was bright and fine, with detached clouds, and a light N.W. breeze. The barometer (aneroid) at 9 p.m. was 30·36, therm. 14°·7."

Thus far Col. Bulger; and now we have only to wait the arrival of Australian meteorological reports to ascertain whether south responded to north, as well as west to east, on the occasion of that remarkably isolated auroral display, abundantly observable, yet observed by so very few persons, in this country on March 17 last.

PIAZZI SMYTH

15, Royal Terrace, Edinburgh, April 26

The Antiquity of Oceanic Basins

I AM much obliged to my friend Prof. Alex. Agassiz for reminding me that his distinguished father, when reporting on the deep-sea dredgings carried on by the United States Coast Survey in 1866-68, explicitly endorsed the views previously put forth by Prof. Dana (to whom, however, he made no reference) as to the geological antiquity of the American Continent and the probable determination of the general outlines of the present Continental elevations and Oceanic depressions at the very beginning of the formation of inequalities upon the Earth's surface.